OUR BOOK SHELF

Practical Botany for Elementary Students. Introductory to the Systematic Study of Flowering Plants. By D. Houston, Science Master, South London Middle-Class School Association. (London: W. Stewart and Co., 1881.)

THIS book differs from Mr. Bettany's (vol. xxiv. p. 235) in being less general in its treatment. It is in fact a series of studies of the coarse anatomy of a number of common plants much on the plan first given in Huxley and Martin's "Elementary Biology." Each study is followed by a technical description, notes on the distinctive characters of a few allied plants, and some miscellaneous matter. "The plants selected are well-known and easily-procured types of the fifteen natural orders included in the Syllabus of the First Stage of Elementary Botany issued by the Science and Art Department, as it is believed that no better selection of natural orders, intended as introductory to the study of classification, could possibly be made." As far as can be judged without working through it, the book is well done, and will be a valuable aid to the teacher if honestly used. Mr. Bettany's plan of teaching the art of describing is soundest. His book can hardly be abused, while with Mr. Houston's there is the risk that incompetent teachers may make their pupils simply learn a large part of it by heart, on the chance of one or more of the plants being set in an examination. Occasionally, where the author abandons the sure ground of personal study, he makes slips. Thus more than half the short list of exotic genera of orchids has the names misspelled. The distribution in time and space of the several orders illustrated is given, though somewhat meagrely. Perhaps in the present state of palæophytology the former is not very important. Under *Orchidaceæ*, for example, the Distribution in Time is given as "not represented," which, apart from the fact that it is a contradiction in terms, means nothing more than that fossils referable to this group of plants have not been found, and are perhaps not likely to be. It may be asked, too, what is the value of the evidence upon which the liliaceous genus *Yucca* is dated back to the Trias?

Von aen Umwälzungen im Weltall. Von Rudolf Falb (Vienna, Pesth, and Leipzig: Hartleben, 1881.)

THIS work is divided into three parts, with separate headings: (1) In den Regionen der Sterne; (2) Im Reiche der Wolken; (3) In den Tiefen der Erde. The author, whose name has frequently been mentioned in our columns, has lectured in various German cities on volcanic and cosmological phenomena, and eventually went to South America in order to study the great volcanoes of the Cordilleras. He conceived an earthquake theory, and his South American friends induced him to publish it. The result is the book now before us, which was published in Spanish at Valparaiso as far back as 1877 ("Estudio sobre los templores de tierra fundado en la historia del universo"). After a sojourn in South America extending over three years, Herr Falb returned to Europe, and his earthquake theory was frequently mentioned in the press in connection with the Agram earthquakes. The theory is simple enough in itself, and the author has at least the credit of being most enthusiastic in its support and in adducing as many facts in proving the same as can possibly be found. Whether he succeeds in proving it is another question. According to Herr Falb's view, all earthquakes, or at least by far the larger majority of earthquakes, are of a volcanic origin; or, to express it concisely, "earthquakes are subterraneous eruptions." The basis of this theory is naturally the supposition that the whole interior of the earth is an ocean of incandescent matter. This is affected by the attraction of sun and moon in exactly the same manner as the sea and atmosphere are acted upon. The second division of the book therefore represents sun and moon as the generators of storms and tides; and in the third division, the principal one, we see the cause of earthquakes traced to the influence of sun and moon. There is no doubt that the author has a special gift of representing his subject clearly and popularly; his eloquence keeps the reader interested from the first line to the last. He quotes no less than thirty facts, from which he draws thirteen different inferences in proof of the volcanic nature of earthquakes. We regret that space does not permit us to enter further into details, but we can heartily recommend the book to our readers.

The Quantitative Estimation of Phosphoric Acid. By M. H. Joulie. Translated by J. Barker Smith. (Dulwich: Published by the Author, 1881.)

THE laudation of M. George Ville, with which the translator opens his preface, discouraged us at first from further perusal of this pamphlet on the citro-uranic method of determining phosphoric acid in manures; and when we did peruse these sixty pages, our chief impression was derived from the comical literality with which French idioms had been rendered into English words. But after all a good many useful hints may be gathered by practical analysts from this little book. Of course most agricultural chemists are familiar with the difficulties which beset the fair sampling and preparation of manures for analysis, and they are also acquainted with many special contrivances for overcoming these difficulties. But information as to new and improved methods of operating, and as to modifications of old processes, is always acceptable.

The essence of M. Joulie's method consists in the precipitation of the phosphoric acid in a prepared solution of a manure by means of a solution containing citrates of ammonium and magnesium. The precipitate which forms is thus produced in the presence of the lime as well as of the iron and alumina of the original liquid; we should like further proof that the whole of the phosphoric acid is invariably precipitated under the conditions described by M. Joulie, especially as he directs the solutions, if rich, to be kept no more than two hours before the ammonio-magnesian phosphate is filtered off. The second and final stage in M. Joulie's method is the solution of the precipitated phosphate and its titration by a standard solution of uranium nitrate.

The second part of this pamphlet describes the treatment of manurial phosphates with solutions of ammonium oxalate and ammonium citrate in order to determine their "relative assimilability." We are not aware that M. Joulie was the first to employ these reagents in the analysis of phosphates—his announcement of the use of the oxalate being in 1872, and of the citrate during the next year. Anyhow, we must demur to some of the conclusions which M. Joulie draws from his experiments, nor can we accept as satisfactory the final directions for the "assay of superphosphates" with which the last fifteen pages of his manual are occupied. The determination of the phosphoric acid and phosphate dissolved by distilled, or, if you will, carbonated water, from a superphosphate cannot be safely replaced by a determination of the phosphates soluble in ammonium citrate. For we lack proof that retrograded phosphates are equal in value with monocalcic phosphate, which alone possesses an initial diffusive power when it is introduced into the soil.

The Butterflies of Europe. Illustrated and described by Henry Charles Lang, M.D., F.L.S. Part I. (L. Reeve and Co., 1881.)

WE have received Part I. of this work, the approaching publication of which was announced in these columns a few weeks back. The whole of the species (and some prominent varieties, &c.) inhabiting Europe proper will occupy about twenty monthly parts, each containing sixteen pages of text and four coloured plates. The plates

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are chromolithographed from the author's own drawings, which appear to be exceedingly well done. We defer a more extended notice until more parts shall have appeared, especially because the subjects illustrated in the first part are almost the least difficult for the chromolithographic process. The text is clearly printed, but a little more care in writing the short descriptions should be exercised. Thus at the very commencement we read as one of the characters of the family *Papilionidæ*, "Larva cylindrical, not spiny, furnished with two retractile tentacles on the second segment." We doubt if this is correct for *all* the European species of *Papilio;* it certainly is not so if exotic species of the same genus are considered; and almost immediately afterwards the author, in defining the genus *Thais* (one of the *Papilionidæ*), says, "Larvæ armed with spines." Nowhere do we find any reference to the veining of the wings, which certainly should have formed part of the sketch of the principal groups given in the Introduction. The author will do well to consider the importance of this suggestion. We presume the chief object of the work is to enable collectors of European butterflies to name their captures, and especially by means of the figures. For this purpose it promises to be exceedingly well adapted.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Chemical Equivalents

MR. J. P. O'REILLY'S paper in your last number (p. 274) appears to involve a complete misconception of the theory of chemical equivalents. The equivalents are mere ratios, and are not altered by multiplying their representative numbers throughout by any factor, whether π or any other.

In fact we may write the equivalents of hydrogen, carbon, and oxygen as x, $12x$, and $16x$, without troubling ourselves about the value of x. This is not only the theoretical view, but the one actually used in practice. So far, there is nothing new or special about writing, as Mr. O'Reilly does, $x = \frac{1}{3}\pi$.

But then Mr. O'Reilly goes wrong, and gets results which contradict his hypothesis. When he writes $H = \frac{1}{3}\pi$, and $O = 5\pi$, the real inference is that the equivalents of H and O are as 1 : 15, instead of as 1 : 16 which he started from.

If his π-values are to be taken as corresponding with the equivalents, this simply means that the latter are not to be depended upon within a limit of error of 5 per cent. I think the mistake is not in this respect, but in overlooking the circumstance that the chemical equivalents are not absolute values, but ratios.

July 24 C. W. M.

Slow Lightning

HAVING just seen the statement of Prof. Tait (NATURE, vol. xxii. p. 341) quoted, as a final authority, against the possibility of distinguishing the source from the termination of a lightning flash, I wish to record a storm that I saw. On May 19 there had been a brisk, hot south-west wind blowing at Gizeh, off the Libyan Desert, at about or over 100° F.; at near sunset a north wind began to come up against it, and there was heavy thunder and lightning all along the line of the mingling of the winds, extending as far as I could see to east and west, and passing a few miles to the north of the Pyramids: the lightning was solely between the clouds, at a height of about one and a half miles; the air around me was 94°, though almost dark. I sat on a rock in front of the door of my tomb (from which I could see eighteen miles over the Delta) and quietly watched the lightning. To my sight there were distinctly differences in the duration of the flashes: some appearing instantaneous and others in which I could see a spot of light occupying an appreciable interval to travel from one cloud to another; and I should be puzzled to draw a hard and fast line between the classes. Does this moving spot-lightning merge insensibly into the variation, of which I saw a fine case years ago near Guildford, where a spark would slowly sail down in the air and then move over the ground before it disappeared?

In any case can these slow flashes (lasting perhaps half a second), seen as well as instantaneous flashes, be disposed of by that blessed word *subjectivity*, which is so comforting to theorists on many objects? Or may not the confession of our ignorance of the cause of ball-lightning be extended to slow flashes in general, instead of treating them just as meteorites were put out of court a century ago? W. M. FLINDERS PETRIE

Bromley, Kent

[Several instances are recorded by Faraday, Joule, and others of flashes which seemed to last for a sensible time. But they are easily explained by one or other of two *veræ causæ*, viz. (1) oscillatory discharges along the same path, succeeding one another at smaller intervals than one-seventh of a second; or (2) phosphorescent matter in the track of the flash. More definite particulars would be necessary before one could decide which was active in the present case.—ED.]

Thought-Reading

As having a bearing upon the hypothesis that in "thought-reading" the information is transmitted by unconscious muscular exertion, allow me to state a modified form of the experiment I tried in the presence of two or three others with Mr. J. R. Brown, who, a few years ago, attracted considerable attention in various parts of the United States by doing precisely what is related of Mr. Bishop in your issue of June 23 (p. 171). After witnessing experiments of the same kind as those stated by Mr. Romanes and performed under the same conditions, I thought to vary them by using a flexible copper wire as a connecting medium. Selecting one, two or three yards long, I held one end in my hand, while Mr. Brown, winding the other end once or twice around his fingers, held it against his forehead, the wire being all the time kept slack between us. Here evidently there could have been no indications received through muscular movements. Yet in this way Mr. Brown would find things concealed or go to certain points determined upon, though apparently with not quite the same readiness and confidence as when the subject's hand was placed against his forehead. Once he partially failed, selecting, instead of a particular spot on the wall I had fixed my mind upon, a small object near it. The experiment in this form was tried with another as his subject, and with equal, if not better, success. GEO. B. MERRIMAN

Rutger's College, New Jersey, July 11

Optical Phenomena

THE photographic halo phenomena described in NATURE, vol. xxiv. p. 260, seem analogous to some observed by me, and upon which, in the spring, I read a paper (since published in the *Notices*, vol. xli. No. 6) before the Royal Astronomical Society. In this I described that not only the sun's disk and the moon's full and partial phases, but also apertures (of similar shape to these) in the shutter of a dark room, when photographed, were, one and all, surrounded by a strong ring halo not visible to the eye. A correspondent essayed some time since to prove in your journal that this halo only surrounded the moon when at full, but on trial the question proved one of time of exposure; and it now seems pretty clear that whatever may be its form and nature, a very bright object when photographed (especially in relief against a dark ground) is found, if sufficient exposure be given, surrounded on the plate by a halo separated from the object by a dark space. Mr. Cowper Ranyard and others attribute these halos to reflection from the back of the plate, a point on which I have not experimented. The dark spot mentioned in connection with the aperture in the rock is probably a reversal of the brightest light owing to the length of exposure.

In two seconds, with Wratten and Wainwright's instantaneous plates, I have found the sun's image so reversed in a camera landscape, showing as a white spot on the negative and a black dot when printed. J. RAND CAPRON

Guildown, July 23

Symbolic Logic

MR. MCCOLL still expresses surprise at my declining to answer a Yes or No question which he was pleased to put to me in

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